

Now from this process of segregation it must have happened that when "dextro- and levo-protein were simultaneously formed" the two kinds of molecules, differently related to environing actions (say ethereal undulations alike in nature and direction), separated themselves into groups of their respective kinds. It is true that in virtue of the small differences between the two classes of molecules, the minute differential actions of forces upon them might be long in producing their effects; and, further, that the segregation might be impeded by restraining forces. But when we remember that segregations take place in long periods of time even where the restraining forces are very great, as instance the formation of hematite nodules and flints in chalk-formations or of siliceous concretions in limestone, the implication is that the segregation would slowly, if not quickly, take place. And then the molecules of either group would exhibit just that optical activity which Prof. Japp, following Pasteur, alleges can result only from molecules formed by vital action.

I do not draw attention to this truth for the purpose of showing the adequacy of the physico-chemical interpretation of life, but for the purpose of showing the inadequacy of Prof. Japp's argument against it. My own belief is that neither interpretation is adequate. A recently-issued revised and enlarged edition of the first volume of the "Principles of Biology" contains a chapter on "The Dynamical Element in Life," in which I have contended that the theory of a vital principle fails and that the physico-chemical theory also fails: the corollary being that in its ultimate nature Life is incomprehensible.

Brighton, October 12.

HERBERT SPENCER.

Organic Variations and their Interpretation.

I SHOULD like, if you will kindly afford me a little space, to offer a few remarks on Prof. Weldon's presidential address to Section D of the British Association.

The first part of that address deals with the question whether individual variations are fortuitous, *i.e.* occur by chance. It contains a very able and lucid exposition of the fact that the distribution of individual variations is of a similar kind, and is open to the same mathematical treatment, as events which happen by chance. I do not think that any one has denied this. It does not admit of dispute. But it is no answer whatever to the reasoning of those who oppose the theory of Natural Selection. The question is whether a given modification, the degrees of which are distributed among individuals according to what may be called the law of chance, originated accidentally, or as the result of a definite ascertainable cause. To give an illustration. If I plant a hundred or a thousand sunflower seeds in good soil in a market garden, at about equal distances from one another, I get a number of sunflower plants which will not all be of the same size. If I measure their heights, or take their weights, I shall find that these magnitudes are so distributed as to form one of Prof. Weldon's curves. If I take another hundred or thousand seeds from the same sack, and plant them in flower-pots, each 6 inches in diameter and of exactly the same capacity, placing the flower-pots in the same garden, I shall get a number of sunflower plants whose heights or weights will form a curve of the same kind. But the mean height or weight of the second lot of plants will be very much less than that of the first lot. This I know to be true because I have tried it. The distribution of the magnitudes has nothing whatever to do with the cause of the difference in the two cases. That cause is limited nourishment in the second case. Similarly in the progressive modification of animals and plants under natural conditions, the distribution among individuals of the degrees of a character has nothing whatever to do with the question of the cause of the character. When selection takes place, by breeding from the larger or the smaller variations, the mean of a character may be raised or lowered, but the question is whether this can be done without regard to conditions of life or not. In numbers of cases there is reason to believe that it cannot. And there is reason to believe that in numbers of cases the mean of a character can be raised or lowered by the application of definite conditions without any selective breeding at all. I will not attempt to prove this here; all I wish to reason that the word conservation is doubly inappropriate. Conservation connotes a conserving and an act of conserving—conceptions utterly at variance with the doctrine asserted; and it also implies that in the absence of a conserving and an act of conserving, the energy would disappear, which is also a conception utterly at variance with the doctrine asserted.

point out is, that Prof. Weldon's argument does not touch the question.

Still more serious objections must be made to Prof. Weldon's evidence concerning the actual operation of selection with regard to the frontal breadth of *Carcinus maenas*. I do not dispute his measurements, but his interpretation of them, which seems to me obviously and demonstrably unsound. He finds that the mean frontal breadth of the crabs at Plymouth was less in 1895 than in 1893, and less in 1898 than in 1895. I have always held that he courted failure by taking for investigation a character which is known to be undergoing progressive change in the individual during growth. We know that change in the proportions of a crab occur only at the ecdysis. It is, I think, certain that the number of ecdyses depend on age, not on size. Prof. Weldon himself remarks that the estimate of age by size is a dangerous proceeding. Yet for individual variations he compares crabs of the same size, not of the same age. Now the results he finds with regard to the diminution in frontal breadth in terms of total length would be exactly the same, if the growth of the crabs had been less in 1895 and 1898 than in 1893; in other words, if the crabs of the same size had been in these years, on the average older, had on the average passed through more moults; for the older crab has a relatively smaller frontal breadth. Now have I any reason for supposing that the crabs grew more rapidly in 1893, and do I suppose that the increased muddiness of the water in Plymouth Sound caused a diminution in the rate of growth? I do not suppose that mud had anything to do with it, but I have good reason for holding that crabs, like oysters, grow faster and larger when the water is warmer. Here is what Mr. Garstang wrote in 1894 concerning the summer of 1893: "Under the influence of the great heat the temperature of the Channel waters rose continuously, until in August it attained a point unprecedented for a quarter of a century; and it was of the highest interest to observe the effect of this high temperature, and of the prolonged calmness of the sea, upon the floating population of the neighbouring portion of the Channel. Numbers of semi-oceanic forms which rarely reach our shores arrived in remarkable profusion. In June the tow-nets were crowded with Salps, while towards the latter end of August they were almost choked by masses of living Radiolaria." The beginning of the year 1895, on the other hand, was exceedingly cold, and in the summer the water temperature was less than in 1893. It is not certain, as so few crabs were measured in 1898, whether their mean frontal breadth was really less than in 1895. But it is a fact that although last winter was unusually mild, the water temperature off Looe in May and June was lower than in the same months in 1895.

It is remarkable that Prof. Weldon found the change in female crabs was less than in males, and it is difficult to understand why the sexes should be affected in different degrees by an increase in the muddiness of the water. On the other hand, as the males in crabs generally are larger than the females, the former would necessarily be more affected in their growth by temperature.

Next as to the experiment which is adduced to show that the increase of sediment is the cause of the selective destruction of the crabs with greater frontal breadth. The survivors of crabs placed in water with china clay had narrower foreheads. But this merely means that they were on the average older, and the younger specimens were killed first, which is what might be expected. Prof. Weldon believes that the cause of death was the entrance of the sediment into the gill chamber, but it appears that the dead crabs had been in the muddy water, while the living were killed after removal. There is no evidence that the clay entered before death, and any dead crab which had been some time in muddy water constantly stirred would probably have mud on its gills.

Lastly there is the experiment of keeping crabs in bottles for a period including a single moult. At first the crabs with broader foreheads died, and in this case the death is attributed to the putridity of the water. In this case there was no sediment, and putrefaction in the water has the same effect as sediment, a fact perfectly in agreement with the view that under unfavourable conditions the younger die first, but inconsistent with the view that death is due to the greater filtering power of the branchial apparatus in the narrower-fronted crabs. The mean frontal breadth having been decreased by selective deaths before the moult, was found, after the moult, to be greater than that of crabs from the sea of the same size. This again is easily explained by increased growth. The crabs in the bottles were

in warmer water and better fed than those in the sea, and therefore on the average were younger after the moult than a number from the sea of the same size. In fact the diminution of frontal breadth depends not on the size of the crab, but on the number of moults it has passed through, while the size depends on the increase at each moult. A crab which has moulted seven times may be smaller than one which has only moulted five or six times.

Another case is on record which seems to me to afford an exact parallel to Prof. Weldon's. In Darwin's "Descent of Man" is quoted the evidence of a hunter who asserted that in a certain district male deer with a single unbranched antler were becoming gradually more numerous and taking the place of those with normal branched antlers. The district referred to was that of the Adirondack Hills in North America. As the witness in question had hunted deer for twenty-one years, Darwin considered his evidence important. J. D. Caton, however, who for many years made the Cervidae his special study in Canada, particularly investigated this case. He satisfied himself that there was no truth whatever in the evidence above mentioned. *The spike-horn bucks seen and killed in the Adirondacks were all yearling bucks with their first antlers.* In all species of Cervus the horns which first grow are simple pointed unbranched spikes; and to prove the existence of spike-horned bucks as a variety, it would be necessary to show that when they cast their horns they developed simple spikes every year throughout life. No attempt was made to prove this, and Caton describes cases which he observed himself, in which spike-horned bucks of unusually large size, which might have been supposed to be full grown, developed branched horns in the following year.

A final objection to Prof. Weldon's argument may be mentioned. All the crabs on whose measurements he bases the conclusion that the relative frontal breadth of the species in Plymouth Sound has actually decreased within a few years, are small specimens 10 to 15 mm., or about half an inch in length of carapace. He makes no attempt to show that the decrease has occurred in adult crabs. The efficiency of filtration would necessarily depend on the absolute size of the filtering mechanism, not on the relative size, since the size of the particles of mud to be excluded remains the same. A crab therefore which survived in consequence of its narrow frontal region at the size of half an inch, would have no advantage when it was 2 or 3 inches long, as the frontal region would then be absolutely much greater. If the mud then kills the small crabs with a broad frontal region, it ought to kill all the adult crabs without exception.

A simple method of testing the soundness of Prof. Weldon's conclusion, with regard to the crabs in Plymouth Sound, would be to compare the mean frontal breadth of adult crabs from that locality, with that of others collected outside the Sound, e.g. at the mouth of the Yealm, where the water is pure, and at Saltash, where the water is much more turbid. If the sediment in the Sound is really decreasing the mean frontal breadth by a process of selection, that dimension must be greater in clean water, and less where there is still more sediment.

J. T. CUNNINGHAM.

Penzance, September 24.

I SHOULD like to be allowed to make a few remarks upon Prof. Weldon's address to the Zoological Section of the British Association; for it seems to me—very interesting as it is—that it is entirely outside the real question of the evolution of varieties and species of animals.

My contention is that *individual differences*—with which Prof. Weldon is solely concerned—*do not afford the materials for new varieties or species* (I would refer the reader to my paper on "Individual Variations," *Natural Science*, vol. vi. p. 385).

A systematist has to consider differences of "form" as well as, and indeed, he regards it as much more important than, "size" or "number." Prof. Weldon, however, refers only to *size* in crabs and recruits, and to *number* in pigs' glands and petals of the buttercup.

If I understand Darwin's theory of "The origin of species by means of natural selection," an individual has some slight variation or new feature, which is beneficial to it in the struggle for life among other organisms, more especially of the same kind—as a "large population" of the same sort is what Darwin and Dr. Wallace demand—then, such an individual may prove itself best fitted to survive and ultimately establish a new variety, the others dying out in the struggle.

But, for one or more crabs to have a frontal breadth a little less than that of others in a group of the same kind of crab is *no new feature*. It is only an "individual difference," such as all organisms are subject to.

"Number" and "size," to be included in varietal characters must be more pronounced than in the case with the crabs. The extreme lengths of the carapace are given as 10·1 and 14·9 millimetres—i.e. two-fifths and three-fifths of an inch; but between those killed by suffocation and the smaller survivors, the greatest difference lies between 816·17 and 787·36, these numbers being the highest "mean frontal breadths in terms of carapace length = 1000"; so that the difference is 28·81, not 3 per cent.

Upon such insignificant differences the life or death of the crabs is supposed to hang!

But the contention presumably is that the smaller crabs will form a new variety. Will any zoological systematist accept this?

But is it not obvious that if natural selection has been always constantly at work in this supposed way with individual differences among plants and animals, some varieties might be looked for among buttercups and *Carcinus maenas*? Take *Ranunculus Ficaria*, which furnished Mr. Burkill with materials for like observations (*NATURE*, February 7, p. 359, 1895), the petals of which vary much more in number than do those of buttercups. If natural selection has been busy over this species for centuries, how is it that *R. Fic.* remains *R. Fic.* still? for it grows in all sorts of places, favourable and unfavourable. It would be easy to make curves for individual differences for the number of petals, stamens, size of leaves, tubercles, &c., but it would all be a waste of energy as far as advancing any illustrations of evolution. Individual differences come up every year, in spite of natural selection and all its imaginary doings. Moreover similar individual differences occur in the leaves all over one and the same tree and of every kind; what can natural selection do among them?

In fact, no one has ever yet shown that a new species has ever arisen out of individual differences "observed in the individuals of the same species inhabiting the same confined area" ("Origin of Species," 6th ed., p. 34).

The utmost that Prof. Weldon has shown is that, *under abnormal and dangerous circumstances*, which have killed off other kinds of marine animals from the Sound at Plymouth, crabs are dying out too; but that the larger ones (older?) are killed off a little faster, perhaps, than the smaller (Is the orifice to the gills a little larger, so as to allow an easier passage for the mud?). We may compare this with the London fogs in winter, which raise the death-rate of older members of the community.

Prof. Weldon says: "I will show you that in those crabs small changes in size of the frontal breadth do, under certain circumstances, affect the death-rate."

As this is the very kernel of the whole matter—for he quotes Darwin as saying, "the theory asserts the smallest observable variation" (observe Darwin requires a "variation"; but there is none at all in any of Prof. Weldon's four examples) "may affect an animal's chance of survival," one anxiously looked out, on reading the address through, for the fulfilment of this promise; but near the end, all he says is, that the immediate cause of death was suffocation by mud clogging the gills, and adds: "I think it can be shown that a narrow frontal breadth renders one part of the process of filtration of water more efficient than it is in crabs of greater frontal breadth."

This opinion is unfortunately no scientific proof; and it is much to be regretted that he did not give us the grounds for his so thinking.

He only measured the carapaces and frontal breadths, but it is presumable that the legs were proportionally longer in the deceased crabs. The question therefore arises, were they, too, concerned in causing the increased death-rate of those with the bigger carapaces?

Once more, what has all this got to do with evolution? No one will dispute these interesting illustrations of chance—a name for all cases where one cannot trace actual causes, or inductive evidence—and its application to individual differences; which, by the way, Dr. Wallace now regards as "non-specific or developmental characters" (*Fortnightly Review*, March 1895, p. 444), and not leading to new varieties; as he did in his work on "Natural Selection" in 1871.

Natural selection determines what shall survive and what shall die in the universal struggle for life; but it has yet to be shown, that the origin of species has anything to do with it.